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A brief history of usable climate science

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Abstract
Recently, certain members of the scientific community have framed anthropogenic climate change as an invitation to reimagine the practice of science. These calls to reinvent science coalesce around the notion of usable knowledge, signaling the need to ensure that research will serve the needs of those impacted by climate change. But how novel is this concept? A historical analysis reveals that the goal of usability is haunted by Euro-American conceptions of instrumental knowledge dating back to the nineteenth century. Even as climate research institutions have embraced the radical epistemic ideal of usability over the past 40 years, they have clung to older definitions of research that are at odds with its anti-individualist implications.

Keywords Usable · History · Pluralism · Climate change · International Institute for Applied Systems Analysis · International Research Institute for Climate & Society

Can we count on science to fix the climate crisis? Is further science irrelevant? It is the premise of this special issue that these questions need to be reframed. The question is not whether we need *more* science, but *what kind* of science we need. Climate scientists themselves have recently begun to adopt this perspective. Alex Hall says the discipline "can't keep doing what we've been doing" (Hall 2020). Ted Shepherd insists that "the societally relevant question is not 'what will happen' but 'what can we do'? That is, what would be the impact of particular actions under an uncertain regional climate change?" (Shepherd 2019, 1). Sophie Lewis frames climate change as an invitation to "reimagine" not just climate science but "science" writ broad (Lewis 2017). These calls to reinvent science coalesce around the notion of "usable" knowledge.

To be usable means to ensure that research will serve the needs of those impacted by climate change. Perhaps more than any other policy problem, climate change has resisted this move, in part because the global models that capture its planetary-scale effects have not had

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the capacity to predict impacts at politically actionable scales. As recently formulated by the international global change research program Future Earth, usable science means incorporating the knowledge, experience, and values of "users," "stakeholders," and indigenous communities into the process of producing and evaluating research. When the consortium announced this policy in 2012, its leaders proclaimed it a "stepchange in making the research more useful and accessible for decision-makers" (Future Earth 2012). Yet usable science for global change is not as new as Future Earth would lead us to believe. Some have traced it back to initiatives founded by UNESCO and ICSU in the mid 1990s (Castree 2019). Others have described it as an instance of "Mode 2" or "postnormal" science, a style of public-facing research that is said to have emerged between the 1960s and 1980s (Lemos and Morehouse 2005). This essay offers a deeper historical genealogy of the concept of usable climate science. In doing so, it reveals a gap between aspiration and reality in the making of policy-oriented climate knowledge. What follows, then, is an exercise in usable history, an effort to reveal the contingent and often contradictory traces of the past in the present—and to provide clarity for the future.

Section 1 suggests that the goal of usability is haunted by earlier Euro-American conceptions of instrumental knowledge, such as "useful knowledge" and "applied science," terms whose meanings were molded by the historical processes of industrialization and imperial conquest. I argue that these terms carry the implicit assumption that the value of solution-oriented science must be weighed against the value of the scientist's autonomy. Indeed, scientific authorities since the nineteenth century have insisted that scientific progress self-evidently depends on the autonomy of the most gifted researchers. Autonomy in this sense means not isolation but freedom to pursue one's curiosity, unhampered by external direction. As reflected in university tenure systems today, freedom of research is typically seen as a prize accorded to a narrow elite of "pure" researchers, who are imagined to be the victors of a meritocratic competition. Against this background, as I argue in Section 2, the ideal of usable science as it emerged in the 1970s was indeed radical: a mode of knowledge production that, in principle, does not reward individualism. Section 3 traces efforts to institutionalize this ideal for climate science in the 1980s and 1990s. The conclusion argues that even as climate research institutions embraced the radical epistemic ideal of usability, they often clung to older definitions of "research" that were at odds with its anti-individualist implications. Recognizing these vestiges of the past is a first step towards truly "doing science differently."

1 Utility and freedom

The goal of "usability" echoes a much longer quest for "useful knowledge," a quest that has been intricately entangled with the histories of capitalism, imperialism, and industrialization. Knowledge that could be *put to use* was the priority of the Baconian reform of natural philosophy in the seventeenth-century England, including Bacon's "Natural and Experimental History of Winds." The ultimate goal of science was no longer certainty, as in the Aristotelian tradition, but instrumentality, or "fit to purpose" (Dear 2006). Useful knowledge was by no means a European invention; it was equally the goal of scientific inquiries in Ming China and the Ottoman Empire (Küçük 2017; Schäfer 2011). In all these cases, projects of useful knowledge bestowed new significance on knowledge associated with the crafts and agriculture. But they did not raise the menial social status of craftsmen and farmers, whose knowledge was simply appropriated by elite scholars.

In the course of the Enlightenment, meteorology lost touch with its roots in practical endeavors (Jankovic 2001). In 1839, the poet and critic John Ruskin described meteorology as little more than a wealthy gentleman's leisure activity, focused on curious, unusual, or wondrous events (Ruskin 1839). Ruskin, a member of the London Meteorological Society, called for meteorology to become responsive to the needs of the common people. And so it did: in the second half of the nineteenth century, national weather bureaus began to collect agro-climatological data and took advantage of the telegraph to issue storm warnings (Coen 2020).

Beyond meteorology, "useful knowledge" was becoming a rallying cry of liberal reformers in industrializing Europe, as in the "Society for the Diffusion of Useful Knowledge," founded in London in 1826. In its new industrial incarnation, "useful" no longer referred to what elites might learn from manual laborers. Instead, it designated a regimen that would ostensibly teach the working class to think for themselves, to give them a taste of intellectual freedom. In reality, the goal was to render workers "useful" to capitalist society, i.e., productive and compliant. As the social theorist Sara Ahmed argues, evaluating knowledge according to the criterion of *use* tends to reinforce existing inequalities: use signals pre-existing habits, "the usual," what one is "used to" (Ahmed 2019). Nineteenth-century scientists also used utilitarian arguments to defend morally questionable research practices, such as vivisection and human experimentation. They even developed theories of mind that naturalized a hedonistic calculation of pleasure versus pain (Gere 2017).

At the same time, however, liberal men of science were wary of demands for utility. In their eyes, the question was how to pursue instrumental knowledge without sacrificing the rightful freedom of the white male upper-class citizen. The Cambridge mathematician Charles Babbage, for instance, advocated the "manufacture" of useful knowledge by factory-style teams of calculators, supervised by machines as a "check…against the inattention, idleness, or dishonesty of human agents" (Schaffer 1994, 209). Such was the hierarchical solution instituted in astronomical observatories of the nineteenth century, which served both the practical needs of an industrializing society (navigation and time-keeping) and the theoretical interests of elite men of science (the structure and composition of the cosmos). While an observatory director enjoyed the freedom appropriate to the work of deducing the laws of the universe, the human computers who worked under him translated those laws into useful knowledge via rigidly disciplined, rote operations. From the perspective of authorities like Babbage, to be constrained to produce useful science was a necessary check on the freedom of those who failed to compete successfully for the privilege of pursuing truth without constraint.

The value placed on free intellectual inquiry in nineteenth-century Europe reflected in part the shift to state-funding of science and the accompanying accountability of scientists to what was coming to be known as public opinion. In early modern Europe, natural philosophers had considered it an honor to be beholden to a powerful patron. For a scholar like Bacon or Galileo, to serve the practical interests of the Tudors or Medicis was to demonstrate the gentlemanly virtue of loyalty (Biagioli 1993). With the rise of state-sponsored science in the nineteenth century, however, scientists became accountable not to princes but to parliaments. The ideal of the freedom of research emerged in opposition to the felt constraint of service to the industrializing state. As founding director of the German Empire's first state-sponsored industrial laboratory, Hermann von Helmholtz articulated this quintessentially modern conflict between the demands of industry and the autonomy of the researcher (Cahan 1989). Like many who achieved fame in natural science in the nineteenth century, Helmholtz had made a personal journey from youthful preparation for a practical profession—in his case, medicine—

to the privilege of free intellectual inquiry as one of a rarified cadre of professional scientists. Thus, it disappointed him to find that even in the section of his institute dedicated to "pure" physics, the staff was "fully occupied with the tasks dictated to her by the engineers" (Rechenberg 1987, 394). Likewise, the astronomer Simon Newcomb, employed by the US Naval Observatory, took it as a "law of development" of modern science that "scientific discoveries are never made by men having any practical object in view…as if Nature persistently refuses the knowledge of her secrets to those who seek them from any other motive than the love of truth" (Newcomb 1874, 294, Lucier 2012). This "law" stood in conflict with the equally apparent fact that the progress of science depended on empirical observations—that is, on "the phenomena and everything connected with it [sic] which can influence the material interest of mankind." In order to reconcile these conflicting demands, Newcomb asked that the public accord scientists "something in the way of consideration which may partially compensate them for devoting their energies to tasks which, from their very nature, can bring them no pecuniary compensation. This reward must be proportioned to merit" (Newcomb 1874, 307). Thus, the hierarchical and putatively meritocratic structure of publicly funded science was meant to optimize the degree of intellectual freedom accorded to each scientific worker. From the perspective of scientific authorities like Helmholtz and Newcomb, producing useful knowledge meant placing just the right degree of constraint on the researcher, calibrated to his "merit." Too much freedom and the research would lose its salience; too much constraint and nothing new would be learned. Few nineteenth-century commentators denied that some degrees of constraint were necessary for the public good, but even fewer questioned that the greatest privilege was to be granted independence.

With the twentieth century came a series of new categories of instrumental knowledge. "Applied" science was promoted by engineers at the turn of the century as a means of raising the status of their profession. "Industrial" science was more specific, addressing problems of manufacturing. "Planned" science took root in the Soviet Union and was admired by Marxist scientists around the world. "Regulatory" science followed in the 1970s, referring to research designed to assess risks to public health and the environment. Each of these inventions in turn lent new urgency to the notion of "pure" or "basic" science, and each portrayed the threat to the scientist's autonomy in a new form. When contrasted with "applied" science, for instance, "pure" science implied freedom from commercial interests. When contrasted with "planned" science, "pure" science instead denoted freedom from ideology or state control. Despite these shifts, the assumption persisted that instrumental value came at the cost of a higher good, the autonomy of the researcher.

Consider the example of the 1959 symposium sponsored by the AAAS at which leading figures in US science considered whether the government was lending sufficient support to "basic science." The director of scientific research at Bell Labs, W. O. Baker, echoed the nineteenth-century liberal conception of freedom when he described the predicament of the modern scientist as a "paradox of choice." The paradox arose from "the way man's mind works in his search for new knowledge and understanding," Baker explained. "In that search, experience shows that the best scholar does just what he wants to do when and how he wants to do it. He is disciplined, of course, by his own will. However, the man working for practical ends, especially in collaboration with a group of others, must obviously adjust his thinking and acting to the common objectives and to the ways agreed upon to advance the group toward those objectives." How then, Baker asked, to "resolve the paradox" that "those having the ablest and most creative minds will prefer to use them in basic research by following up the undirected, uncontrolled, unspecified, unprogrammed, and certainly unknown courses revealed as the work itself goes ahead"? Baker made clear that industrial laboratories needed to be designed in order to foster *the illusion of choice*. It was essential "somehow to present to the gifted researcher situations in which he will feel little or no inhibition of the free travel of the pathways of his mind" (Wolfle 1959, 68-9). In other words, freedom of research would be replaced by the *perception* of freedom of research. This would be accomplished by such "devices of modern technical organization" as "having development and engineering clearly separate from, but near to, research." Forty years later, the political scientist Donald E. Stokes would echo this triumphant account of industrial science in his widely cited text *Pasteur's Quadrant*, where he celebrated the power of scientific institutions like Bell Labs to "encourage…research that is both basic and use-inspired" (Stokes 1997, 81).

As Naomi Oreskes has recently demonstrated, Cold War-era geoscientists in the USA eagerly embraced this illusion of choice. Oceanographers saw no contradiction in describing the research they performed with funding from the Navy as "basic science," as if their sense of curiosity just happened to align with military needs. Even while solving problems of submarine navigation, they repeated the mantra that "autonomy is a necessary element of science" (Oreskes 2021, 648). An analogous argument might be made about meteorologists in the postwar USA. In the 1950s, meteorologists were reinventing themselves as "atmospheric scientists" intent on discerning the physical laws of the atmosphere (Fleming 2016). Yet much of their research agenda was shaped by the military's quest for weather and climate control (Hamblin 2013). As Heather Douglas argues, many US scientists in the second half of the twentieth century interpreted academic freedom to mean that they were free to disregard questions about the uses of their research (Douglas 2021).

Indeed, the quest for *usable climate science* arguably began not with meteorologists but with a political scientist. In the 1970s, scientists weren't yet seeing a clear signal of anthropogenic global warming in their surface temperature data, but they were seeing the effects of the El Niño Southern Oscillation or ENSO, an irregular fluctuation in winds and sea surface temperatures that affects much of the tropics and subtropics. The phenomenon was suspected to be a factor in the drought that devastated the Sahel region of West Africa in the early 1970s. Enter Mickey Glantz, a charismatic former engineering student and a newly minted scholar of postcolonial nation-building. In 1974, somewhat serendipitously, he won a postdoc at NCAR. There he embarked on a Rockefeller-funded study of the hypothetical value of "long-range weather forecasts," meaning the possibility of predicting conditions a season in advance. With large populations suffering from drought, such information seemed like it might hold the key to preventing future catastrophes. In 1974, Glantz surveyed a hundred scholars from a range of disciplines with expertise on West Africa: suppose, he asked, you had known a year ago what was in store meteorologically for the Sahel in the coming year, suppose you had had a perfect forecast, what would you have proposed to do differently? Glantz received plenty of suggestions for how locals *could in principle* have adapted to the unusually low rainfall, such as storing water, culling livestock, establishing a grain distribution network, or even shifting populations. But from a practical perspective, Glantz judged that few of these measures were feasible, given local social and political conditions. He sent the results of his study to the Bulletin of the American Meteorological Society, but to his surprise, reviewers were hostile. The message was that his study had nothing to do with meteorology; meteorologists had no reason to care how their results were *used* (author's interview with Michael Glantz, 26 January 2021; Glantz 1977). Nonetheless, Glantz eventually managed to get his study published in BAMS, and it demonstrated a crucial point: climatic information may not be useful to the people most vulnerable to climatic change.

From "useful knowledge" in the early nineteenth century to "applied" and "industrial" science in the twentieth, categories of instrumental knowledge have reflected both the priority of the individual freedom of the researcher and the tendency of utilitarian values to reinforce the social status quo. As we will see in Section 2, the idea of "usable" science built on this long legacy of attempts to categorize and theorize forms of instrumental knowledge. Yet it introduced the potential for a radical break with this history.

2 The usable turn

The concept of usable science, as it was incorporated into climate research in the 1980s, was part of an emerging discourse around "the science of science." This conversation included a range of disciplines: history, philosophy, and sociology of science, as well as studies of innovation and knowledge exchange by political scientists, anthropologists, and geographers. Science studies in this sense were also strongly influenced by politically radical scientists of the 1960s and 1970s. Indeed, while sociologists and historians were at work describing what Thomas Kuhn famously called "normal science" (Kuhn 1962), left-leaning scientists were already calling for a new, more democratic mode of scientific practice, one more appropriate to an age when questions of science policy tended to be high-stakes and low-certainty. Retrospectively, in 1979, the Austrian sociologist of science Helga Nowotny termed this phenomenon "critical science." She judged it all the more powerful because it came "from within" (Nowotny and Rose 1979, 22).

Nowotny was among the social scientists beginning to work closely with natural scientists on questions of policy, including climate policy. Elite organizations like the National Research Council in the USA were developing an expert-led, consensus-based approach to synthesizing or "assessing" science for policy-making (Oppenheimer et al. 2019). This dominant approach was premised on the separation of facts from values, insisting on the objectivity of the experts involved and the demarcation between science and policy. It was understood as a discrete step in a temporal sequence: research, assessment, policy-making. The aspiration to "usable" science broke with this paradigm in every respect.

The concept of usable science built on the post-positivist turn in History & Philosophy of Science. From theorists like Kuhn, Lakatos, and Feyerabend, climate researchers and policy analysts alike took the lesson that scientific conclusions rarely hinge on direct empirical observation. For the field of policy analysis, this meant a sharp turn away from the hyper-rationalism of World War II operations research, and instead an embrace of the "craft" aspects of policy-making. According to this revisionist view, if there was any rationality to be found in political decision-making, it entered as a post-hoc reconstruction of a process that was, in reality, characterized by "intuition" and "interaction." But anti-rationalism was not the only radical feature of usable science.

As I argued in the previous section, efforts to increase the utility of science had long been said to hinge on optimizing the degree of constraint on the researcher while creating the illusion of freedom. Here, I propose that theorists of usable science were beginning to think outside of the freedom-constraint axis. In doing so, they echoed the participatory research movement of the 1960s and 1970s, which originated in postcolonial critiques of Western economies (Guldi 2021). Participatory research referred to a loosely linked global network of activists who were designing information infrastructures to empower members of marginalized communities to engage in bottom-up social change. These activists did not hide the fact that participatory research meant (in the words of the Indian engineer and participatory theorist Rajesh Tandon) the "loss of complete control by the researcher", and they did not try to restore the *illusion* of control (Guldi 2017).

This shift is visible in the influential writings of nuclear physicist Alvin Weinberg, who promoted policy-oriented climate science as the founder of the Institute for Energy Analysis at Oak Ridge. Like Baker, Weinberg wrote of the core challenge of postwar science as the "problem of choice." Historians have tended to see Weinberg as yet another scientist anxious about losing autonomy to "Big Science," a term he coined. But for Weinberg, in contrast to Baker, the point was not to foster the illusion of intellectual freedom while nudging scientists towards useful research. Instead, Weinberg raised the question of the relationship between "Scientific Choice and Human Values":

> *Merit is to be judged by the degree to which the activity, scientific or human, contributes to the unity and illumination, and ultimately to the harmony of the many activities with which it interacts*. Thus, there is a kind of ethical reciprocity: we decide on the good from the standpoint of the neighboring universes; in making the judgment, we ask: Does the activity or attitude we are judging help create a unity, a harmony in the universe doing the judging? (Weinberg 1967, 121)

In this explicitly anti-individualist vision, Weinberg was advocating the assessment of science according to the power of new knowledge to coalesce a *community* around it. As we will see, his imagery of the multiplication of viewpoints would find resonance in early accounts of the practice of usable climate science. In 1972, Weinberg introduced a new keyword, "trans-science," which referred to research "at the interface between science and politics," concerning value-laden questions that science alone cannot resolve. Climate change became a prime example, and the IEA became a center for the study of climate change policy options.

For researchers addressing climate change, the most influential formulation of the "usable" ideal came in a 1979 book by the political scientists Charles Lindblom and David Cohen (author's interview with William C. Clark, 15 May 2019; email from Michael Glantz to author, 25 April 2021). Lindblom had been disillusioned by an early stint at RAND and remained a critic of rational choice approaches to politics for the rest of his career. In the 1970s, it became clear to him that his career-long commitment to the polycentric ideal of political pluralism [1] was fundamentally undemocratic, incapable of challenging America's economic and racial hierarchy. Impassioned by this realization, Lindblom launched a tirade against his own discipline. He attacked its positivism and hyper-rationalism, and he urged his colleagues to recognize the value of the situational knowledge of ordinary citizens. Social problems could more often be solved with greater legitimacy through unpremeditated "interactions" among stakeholders than by means of expert interventions. "Numerous forms of human interaction… have…the effect of reducing a social problem, thus achieving an improved outcome. They are thus *alternatives to understanding*, *thought*, *or analysis* as a method of reaching a 'solution'" (Lindblom and Cohen 1979, 25). Unlike human interaction as imagined by game theorists at the time, "interaction" in Lindblom's sense was not the outcome of individuals behaving in predictably rational ways. Here, Lindblom was inspired in part by post-positivist Philosophy of

[1] In political science, pluralism is the theory that democracy is best served by competition among interest groups. This is to be distinguished from scientific pluralism, the principle that the world cannot be described by any single unified theory and that science therefore needs a multiplicity of methods and epistemic frameworks.

Provider	Documents
JSTOR	16,136
Portico	33,252
Chronicling America	538
Reveal Digital	62
Total	50,000

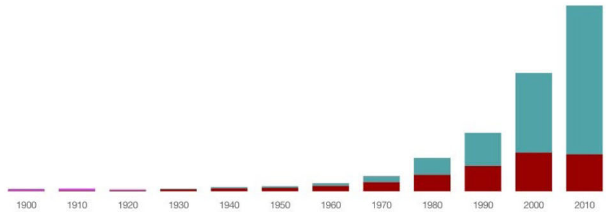

Fig. 1 Publications mentioning “usable science” or “usable knowledge” by decade across JSTOR databases

Science, above all by Imre Lakatos’ proposal that science be evaluated not according to the agreement of theory with evidence, but according to the generativity of the research program.

As Figures 1 and 2 demonstrate, references to “usable” science rose after the publication of Lindblom and Cohen’s book in 1979, and applications of the concept to climate change have been growing exponentially ever since. As Mickey Glantz recalls, “the book by Charles Lindblom and David Cohen offered me a succinct way to capture my view that not all knowledge is derived by a scientific method” (Glantz email to author 25 April 2021). However, not everyone appreciated the more radical elements of the theory of usable knowledge. It departed from earlier ideals like useful knowledge, applied science, and planned science because it requires knowledge to be assessed not in hindsight but rather in media res. Research and assessment continually feed into each other, with the criteria of evaluation coming from users, and focusing, of necessity, on process over product. Usable science thus broke with other trends of the 1970s: it was neither retrospective, as in NRC-style scientific assessments, nor futurological, as in the culture of prediction that thrived at RAND, awaiting confirmation or refutation by future events. Rather, usable science focused on what was present and immediate: the stakeholders already in the game, the problems already named, the solutions available. Like participatory research, usable science was not the product of individual cognition but of collective experience. It was not a representation of the state of the world but an intervention into it. Arguably, no participant in the process had a greater claim to “authorship” than any other. Usable science did not depend on the “illusion of choice,” on constraints masked as freedom, but rather on an enthusiasm for collective experience that bordered on the mystical (Kelty 2020).

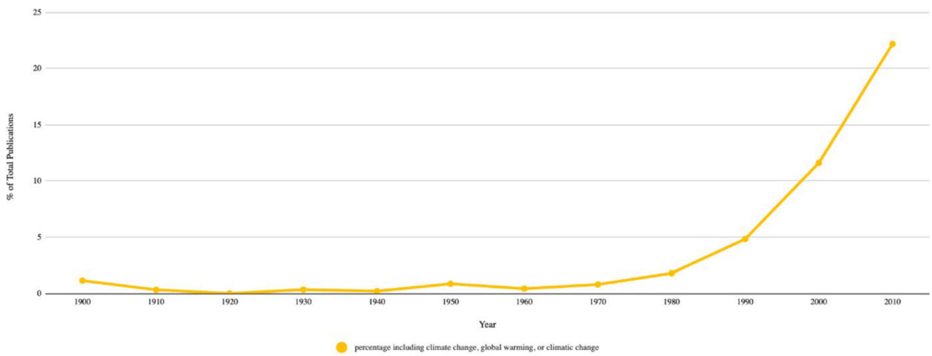

Fig. 2 Percentage of publications represented in Figure 1 using “climate change” or “climatic change” or “global warming”

Theorists of usable science did not claim that this approach was epistemically superior to an expert-driven, retrospective, consensus-based assessment. They argued only that in certain circumstances, "the consensual mode of synthesis is impossible or inappropriate. Unfortunately, this is precisely the case for many of the value laden, incompletely understood issues that arise in the evaluation of strategies for a sustainable development of the biosphere" (Clark and Munn 1986, 38). In these cases, producing usable knowledge was the best one could do—and scientists had to learn how to do it.

3 Producing usable climate science

In short, usable science for global change is not as new as Future Earth has claimed. In fact, it is contemporaneous with the formulation of the "carbon dioxide problem" as such in the late 1970s. Here, I can only begin to sketch the 40-year history of efforts to make climate research usable. Usable climate science has thrived primarily at extra-academic institutions, for reasons already implicit in the judgment cited above that expert panels were "inappropriate" in situations of high risk and low certainty. (Exceptions include the "extension" arms of the US land-grant universities, as described in Robert Kopp's article in this issue, as well as centers at universities such as Arizona State University, Harvard, and the University of East Anglia.) Table 1 lists the most important of these sites in order of founding.

These institutions were intentionally designed to foster what scholars in the early 1990s were coming to call "epistemic communities" (Haas 1992). Indeed, some of these institutions served as exemplars for the emerging concept of the "boundary organization," a site where science meets policy (Guston 2001). The meanings of such terms as "epistemic community," "boundary organization," "co-production," and "usable science," as they pertain to climate change, have taken shape in part through reflections on the work of these institutions.

Yet their institutional cultures differed widely. Consider the examples of the International Institute for Applied Systems Analysis (IIASA) and the International Research Institute for Climate & Society (IRI). IIASA was founded in 1972 as a bridge-building institution between scientists on either side of the Iron Curtain. Its mandate was to apply systems analysis (a quantitative-rational approach to decision-making, derivative of wartime operations research) to apolitical problems of international significance (Rindzevičiūtė 2016). The institute was housed in an abandoned eighteenth-century palace in Laxenburg, just outside Vienna, surrounded by woods where the nobility had loved to hunt. Despite the stately facade, the

Table 1 Major institutions sponsoring usable climate science for international users. Note: NOAA's Regional Integrated Sciences and Assessments (RISA) program was founded in 2000 to generate usable climate science for regional users and currently has 11 centers throughout the USA

Institution	Location	Founded
National Center for Atmospheric Research (NCAR)	Boulder, Colorado, USA	1960
International Institute for Applied Systems Analysis (IIASA)	Laxenburg, Austria	1972
Institute for Energy Analysis (IEA)	Oak Ridge, Tennessee, USA	1974
Tellus Institute	Boston, Massachusetts	1976
Third World Institute	Montevideo, Uruguay	1989
Stockholm Environment Institute	Stockholm, Sweden	1989
Wuppertal Institute for Climate, Environment and Energy	Wuppertal, Germany	1991
International Research Institute for Climate and Society (IRI)	Palisades, New York, USA	1996

institute had a rollicking atmosphere, well lubricated with Austrian wines. The atmosphere at the IRI was more earnest. Founded in 1996, following six years of negotiations with the National Oceanic and Atmospheric Administration, its motto is “Science NOT for science’s sake.” A short drive from upper Manhattan, it lies on the campus of Columbia University’s Lamont Doherty observatory, a compound that mixes rustic cabins and new LEED-certified architecture. The building design features large open spaces for conversation and a linear layout that forces colleagues to pass each other frequently. Discussing the institute’s “mission” over brown-bag lunches was a mandatory ritual.

At IIASA, usable climate science grew out of the energy crisis of the 1970s. Energy policy was a field in desperate need of usable science, and IIASA set out to provide it. On its staff was the ecologist Bill Clark, who also worked at Weinberg’s Institute for Energy Analysis (alongside the political anthropologist Steve Rayner, later a staff member at IRI). IIASA researchers took a leading role in organizing the earliest international conferences on the social implications of global warming, beginning with the 1978 meeting at IIASA “Climate, Carbon Dioxide, and Society.” In 1982, Clark published *The Carbon Dioxide Review*, in which he cited Weinberg and Lindblom and Cohen on the need to do science differently when faced with urgent social concerns. Against the reigning model of consensus-based scientific assessment, he underlined the “need for open, critical debate” (Clark 1982). At IIASA, “users” of climate science typically referred to policy-makers at the national or international level.

At IRI, usable climate science grew instead out of attempts to forecast climate fluctuations, in order to alleviate poverty in the Global South. Idealistic scientists hoped to provide information to help tropical nations prepare for climatic variations. Thus, at IRI, “users” typically referred to policy-makers in developing nations.

While IIASA and IRI diverged in their institutional cultures and intellectual missions, their histories display parallels that throw into relief the challenges of institutionalizing usable science.

4 Reflexivity

A first commonality between the institutions was an emphasis on reflexivity: both IIASA and IRI encouraged researchers to reflect on their own practices of knowledge production, to engage in what I have called the “science of science.” Social scientists came to play important roles in this endeavor, despite marginal positions at both institutions.

The IRI’s original plans had excluded social scientists from staff, and the “applications” division was the last to be planned and put into operation. The social scientists were meant to build a communication circuit with “end users” of the institute’s forecasts: they would “translate” the science for the users, study users’ needs and constraints, and feed the latter information back to the physical scientists. The IRI directorship termed this model “end to end,” uniting physical science with its societal application under one roof (Agrawala et al. 2001). Before long, the institute found it needed “help with bridging between disciplines.” As the assistant director for science management put it at the time, the IRI had all the ingredients on the counter, but it needed a Julia Child to mix them properly (David Guston’s interview with Carolyn Mutter, 5 November 1999). In this way, social scientists at the IRI, typically trained in environmental anthropology or development economics, ended up working as Science Studies researchers: they reflexively analyzed the process of producing usable climate knowledge. As Mickey Glantz had foreseen, the institute quickly ran up against the limits of the usability of their forecasts. More fundamentally, they had to confront the ambiguities latent in the institute's idealistic goal, that of science for "societal benefit." As anthropologists on staff demonstrated, different actors brought very different visions of societal benefit, few of which were likely to converge with scientists' own (Broad 1999, author's interview with Kenny Broad 12 March 2021, author's interview with Shardul Agrawala 10 March 2021).

At IIASA, Clark and Holling allied with social scientists in pursuit of reflexivity. In 1975, Clark circulated a paper among IIASA staff titled "Looking at Ourselves," in which he chided his colleagues for their "aversion to mirrors." He urged that the institute makes the critical study of "knowledge systems" part of its mission. Citing Lindblom, he observed that even such a highly technical, "rational" mode of decision-making hinged on value judgments. The "implementation" of decisions was rarely a rational process, as illustrated by debates at the time over the siting of nuclear reactors (Clark 1975). Over the following decade, Clark invited a number of scholars from Science & Technology Studies to IIASA to collaborate on the analysis of knowledge systems, including Helga Nowotny, Brian Wynne, and Jerry Ravetz (e.g. Ravetz 1986). A high point of the reflexive turn at IIASA came in 1984 with a scathing critique of the institute's flagship energy model, promoted as generating plausible scenarios of global energy use fifty years into the future, including estimates of CO_2 emissions. In widely circulated articles, Wynne and another IIASA researcher charged that the seven-year study had produced a model that merely "reproduced the input assumptions" (Keepin 1984, 221; Wynne 1984). While some at the time saw their intervention as a fatal blow to IIASA's reputation, others interpreted it as an instance of responsible self-critique and self-correction (author's interview with Brian Wynne, 2 February 2021).

5 The limits of prediction

Second, both institutions came to recognize that predictive modeling had limited value for usable climate science, even aside from the epistemic limits to the predictability of the climate system. Prediction was the initial raison d'être of IRI, as suggested by its original name, the International Research Institute for Climate Prediction. In 1986, two atmospheric physicists at Columbia University, Mark Cane and his student Steve Zebiak, announced they had cracked the problem of forecasting ENSO. Their coupled atmosphere-ocean model could produce reasonably reliable forecasts of El Niño events over a year in advance. It was not long before regional climate services were channeling this information to scientists, policy-makers, and farmers in regions where agriculture, fisheries, water resources, and public health were highly sensitive to inter-seasonal climate variability. But all did not go as predicted. The 1986 El Niño materialized according to the forecast, but few people heeded the warnings. Cane and his colleagues had taken a "'If you build it they will come attitude.' That didn't work out." Having his forecasts fall on deaf ears was, he says, a lot like getting negative reviews on a manuscript. "Your first thought is 'What idiots'! There's gold in the streets, why aren't they picking it up. Then you get it and realize have to think harder about how to convey this information and what it means" (author's interview with Mark Cane, 6 May 2019).

Soon social scientists were exploring what had gone wrong and identifying obstacles to acting on forecasts. It turned out, for instance, that water managers in some US cities feared public criticism for responding to El Niño forecasts, or worried that they would have resources cut in non-El Niño years if they did. Subsistence farmers often chose to follow traditional practices rather than incorporating information from forecasts. Government officials sometimes had incentives to ignore forecasts, for instance if they hoped to use food aid to resolve conflict in their region (Dilling and Lemos 2011). IRI researchers also observed that forecasts tended to benefit some segments of a local population and disadvantage others—often in ways that aggravated existing inequities, such as favoring commercial over small-scale agriculture and fishery.

While other providers of "climate services" hawk forecasts on a timescale of decades, IRI scientists have argued that citizens and policymakers in poorer countries rarely have the luxury of planning that far out. They have also criticized other research institutions for pursuing "usability" by means of downscaling from GCMs, without acknowledging the inherent limitations to predictive certainty at the local scale (Nissan et al. 2019). Instead, they stress what can be learned from history, from "past climate variability and trends." Decisions about adaptive measures can often be made on the basis of qualitative conclusions alone, they argue. When users ask for greater precision, they try to "shift the users' purpose" (author's interview with Lisa Goddard 31 July 2019). Far more than a forecasting factory, the IRI has become a hub for networking diverse makers of climate knowledge and reflecting on the character and purpose of the knowledge-making process. Using the language of Science Studies, IRI researchers attribute their successes to the capacity of this "dialog" to foster "legitimacy and trust" (Vaughan and Dessai 2014).

At IIASA, a parallel critique of prediction was spearheaded by the ecologist Buzz Holling, who became the institute's third director in 1981 (Schrickel 2017). In fact, the role of "the unexpected" had been central to Holling's ecological research; most famously, it was at the heart of the influential concept of resilience that he introduced in 1973. Holling had been Clark's PhD advisor at the University of British Columbia, where the two ecologists had learned to work directly with users of ecological research. Holling codified these experiences in his program of "adaptive" environmental assessment (Holling 1978), which resonated with Lindblom's notion of usable knowledge as interaction. Upon becoming director, Holling launched a critique of the direction the institute had taken in its first decade. Much like Lindblom's attack on the RAND institute, Holling complained of a rush to model and quantify problems that were not yet well understood in their social and political dimensions (Holling speech to Ralf Yorque Society, no date, Archive of the Austrian Academy of Sciences, IIASA/Arb. Plan 1982/2).

In the 1980s, influential climate scientists like Steven Schneider and James Hansen still hoped to sway policy by means of more accurate predictions of the global impacts of greenhouse gases (Heymann and Hundebøl 2017). By contrast, most researchers at IIASA treated computer models as mere heuristic tools (Robinson 1981; Häfele and Rogner 1984). Clark and Holling emphasized the need to study climate impacts and policy options at multiple scales, matching those of policy-making, and to do so with participants from a range of countries and disciplines. They developed board games and "policy exercises" as an interface between science and decision-making (e.g., Robinson and Ausubel 1981). At a time when historians and philosophers of science were just beginning to talk about the "disunity" of science and the virtues of pluralism, IIASA was putting pluralism into action. As Clark put it in 1985, "Efforts to develop better critical skills for science with policy implications should aim not for a unique evaluation, but rather for an enhanced understanding of different evaluative criteria on the part of all role players. We most need, in other words, a more sophisticated and sympathetic understanding of the multiple perspectives involved" (Clark and Majone 1985). As at IRI, IIASA researchers downplayed the role of predictive models and foregrounded knowledge-making practices that fostered interaction and dialogue. The priority was to facilitate international and interdisciplinary cooperation: to make the research usable from the start.

6 Precarity

Third, researchers at IRI and IIASA who committed themselves to the pursuit of usable knowledge faced precarious careers. In meteorology, credit and funding tended to flow to work on models, not applications. And cross-disciplinary collaboration presented unfamiliar challenges, whether with Western-trained social scientists or farmers in Ethiopia and Peru. Its rewards were not the usual ones of a publication in *Tellus* or *Geophysical Research Letters*. Those who stuck with it tended to be the ones who found they had a knack for it. As one RISA scientist put it, some of her colleagues just weren't "comfortable going out and talking to people" (Brugger et al. 2016, 359).

At IRI, the "end-to-end" ideal of unifying research from physical theory to social application placed constraints on researchers professionally and personally: it limited their opportunities to publish and win grants, thus impeding promotion to tenure, and it demanded extended travel incompatible with family responsibilities. The irony is that *theorists* of usable science have flourished professionally, while some scientists working directly with users on "applications" have struggled to meet promotion standards.

Jill Jaeger is an example of a researcher who successfully navigated the risks of staking a career on usable science. In the late 1970s, Jaeger (then Jill Williams) led IIASA's early efforts towards usable climate science. As a recent PhD in geography with expertise in paleoclimate modeling, Jaeger was at IIASA to study climate in relation to energy systems. With her background in both physical science and human geography, she found it easier than most scientists to "talk to people from different places." It was Jaeger's view from the start that "scientific quality" alone was not going to solve the climate problem. What was needed was dialogue that could overcome national and disciplinary differences (author's interview with Jaeger 8 August 2018). She describes her role at IIASA—and subsequently at the Stockholm Environment Institute and the Wuppertal Institute—in terms of the work of "assessment":

> It's bringing together information from all different sides and making it useful, in one way or another, for decision making…A lot of what I've been doing has also been facilitation, moderation, and engaging stakeholders in discussions and dialogues about sustainability (author's interview with Jaeger 18 March 2021).

Jaeger succeeded in facilitating this synthesis across disciplines and regions. Yet, this work didn't count as "research." As she put it recently, "I haven't been doing the modeling myself; I haven't actually been doing the scientific analysis. I'm taking that and then making it usable. And that's a continual thing." Jaeger quickly realized that this kind of scientific work was not a ticket to a traditional academic career. Instead, following her time at IIASA, she crafted an alternative career as an independent consultant, leading assessments for the Stockholm Environment Institute and the Wuppertal Institute, among others. This had the advantage of allowing her to set her own hours and work from home while raising her children.

Thus, despite the outward contrasts between IIASA and IRI, researchers at these institutions shared a formative experience: they had seen with their own eyes that making science usable meant bringing into existence a community of users. They agreed that usability depended on diverse participants, a reflexive orientation, and an iterative, inclusive process for evaluating research. Breaking with established modes of scientific assessment, proponents of usable climate science framed the research process as a form of problem-solving through interaction, not rational analysis. In this respect, they converged with STS scholars who were just then concluding that knowledge-making, to be effective, must be an act of social transformation (Jasanoff 2004). In the words of one of the leading theorists of usable climate science, Maria Carmen Lemos, this mode of knowledge production is largely about "just being there in that space" together (author's interview with Lemos 31 March 2021). Whether they phrased the lesson in terms of "sympathetic understanding" or mutual "legitimacy and trust," researchers at IRI and IIASA agreed on the crucial point that, to produce "usable science," the research process had to create a community of users.

Accounts like these might lead one to believe that such a knowledge-making process is self-sustaining. Indeed, Lindblom had portrayed interaction as a spontaneous process, saying nothing of the constant labor of mediating among divergent points of view. As RISA scientists have observed, "The integration of multiple disciplines (beyond academic exercises) and multiple perspectives remains challenging to generate and sustain in practice" (Pulwarty et al. 2009). At the heart of usable climate science has been the work of "assessment" as described by Jill Jaeger above, as well as the editorial work performed by the typically female librarians and administrators who supported the large, multi-author, cross-disciplinary publications of institutes like IIASA and IRI. At both institutions, much of that work took place behind the scenes and without authorship credit.

7 Conclusion

One lesson of this history is that usable science is usable only in favorable political contexts. The program of usable climate science flourished in Europe in the 1980s and 1990s, thanks to institutions like the Stockholm Environment Institute and government-sponsored research in Germany (Cavender and Jaeger 1993). It subsequently shaped the new field of Sustainability Science along with the research agenda of international programs like Future Earth, and it has influenced environmental policy in the European Union. By contrast, in the Republican-dominated USA in the 1980s, the pro-oil agenda made it far more difficult to set science and policy into dialogue (Oreskes and Conway 2010; Howe 2014). The US Global Change Research Program of 1990 called for the provision of "*usable* information on which to base policy decisions relating to global change," yet, this was widely interpreted to mean that scientific research and assessment would be conducted prior to and in isolation from policy analysis (PL 101-606, Sec. 104.b.1, Pielke Jr. 1995). Usable science in the original sense proved too radical a proposition. As RISA scientists have explained, "Program managers needed to outline an acceptable process (to academic partners and federal offices) for experimenting with both interdisciplinary integration and stakeholder engagement." Otherwise put, it is impossible to practice usable science without interested users on hand.

In the face of these political obstacles, even the staunchest advocates of usable climate science have fallen back on older ways of talking about science's utility. They laud institutions like IIASA and IRI for allowing elite scientists to serve society simply by sharing their models. In doing so, they echo the long history of strategies to produce useful knowledge without constraining the freedom of inquiry of the most deserving minds. They also resort to language redolent of the old idea of utility as a function of freedom curtailed, making frequent recourse to the metaphor of "harnessing" science and technology for sustainable development (e.g., Kasemir et al. 2003, xix, xvii, Cash et al. 2003, 8086, 8089, 8090). The metaphor betrays their need to convince potential patrons that use-value can be guaranteed by the proper design and management of scientific institutions. These rhetorical habits display the staying power of the idea that useful science depends on allocating freedom and imposing constraint according to a hierarchy of merit.

This residue of nineteenth-century thought has no place in an institution dedicated to usable science. To see why, we need to recall the meaning that "usable knowledge" carried when scientists first recognized its relevance to the problem of climate change. Usable knowledge is anti-rationalist and hyper-pragmatic. Its validity cannot be established by peer review, but only by the role it plays for a community of users. The necessary conditions for usable knowledge have nothing to do with either freedom or constraint: constraint is irrelevant when there are no rules to follow, and freedom is beside the point when the goal is to forge relational bonds. Collaborative knowledge-making requires an ethos of care and an acceptance of interdependence; it is incompatible with a Manichean liberal view of freedom as the absence of dependence.

In short, the goal of usability alters the very meaning of *research*. In a world of "pure" and "applied" science, research has designated activities along a spectrum from the individualistic pursuit of curiosity to the disciplined performance of routine tasks. In the context of usable science, however, research is primarily a form of care: care for data and its analysis, and care for people and their relationships (https://environmentalenforcementwatch.org/about/, accessed 20 February 2021, Wylie et al. 2017). When the mark of success is not the approval of experts but the functionality of the research community, research is about sustaining the interactions that build trust. Making science usable means institutionalizing research as care.

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